

Reply to the comment on:
“Thermostatistics of Overdamped Motion of Interacting Particles”
 by Y. Levin and R. Pakter

J. S. Andrade Jr.^{1,3}, G. F. T. da Silva¹, A. A. Moreira¹, F. D. Nobre^{2,3}, E. M. F. Curado^{2,3}

¹*Departamento de Física, Universidade Federal do Ceará, 60451-970 Fortaleza, Ceará, Brazil*

²*Centro Brasileiro de Pesquisas Físicas, Rua Xavier Sigaud 150, 22290-180, Rio de Janeiro-RJ, Brazil*

³*National Institute of Science and Technology for Complex Systems,
Rua Xavier Sigaud 150, 22290-180, Rio de Janeiro-RJ, Brazil*

In their comment, Levin and Pakter present an analytical solution for the mechanical equilibrium of a system of particles interacting through a potential given by the modified Bessel function, and confined by a restoring force. This derivation yields a quadratic dependence for the local density of particles, which is consistent with Eq. (14) of our work. Based on this result, Levin and Pakter question our interpretation that a system of overdamped particles at $T=0$ is a physical realization of Tsallis thermostatics with entropic index $\nu = 2$. They claim that the same density profile can be obtained using only Newton’s laws.

In what follows we provide a reply to the remarks of Levin and Pakter. We first show that their attack to our results and analysis is conceptually unfounded and rather misleading. Inexplicably, they simply choose to categorically dismiss our elaborated and solid conceptual approach and results, without employing any fundamental concepts or tools from Statistical Physics. We then demonstrate that the results of Levin and Pakter do not present any evidence against, but rather corroborates, our conclusions. In fact, the results shown in their comment correspond to a confining potential that is 1000 times stronger than the typical valued utilized in our study, therefore explaining the discrepancy between their results and ours. Furthermore, in this regime where higher vortex densities are involved, vortex cores might get so close to each other that can no longer be treated as point-like defects. As a consequence, Ginzburg-Landau equations should be employed instead, meaning that the physical conditions implied by the results of Levin and Pakter should be considered with caution in the context of the Physics of interacting superconducting vortexes.

1) Levin and Pakter claim that our result for the density distribution of particles “has nothing to do with the Tsallis statistics”.

In our letter, we show that the the non-linear Fokker-Planck equation in the form

$$\frac{\partial P}{\partial t} = -\frac{\partial[A(x)P]}{\partial x} + Dq\frac{\partial}{\partial x} \left[P^{q-1} \frac{\partial P}{\partial x} \right], \quad (1)$$

results in a dynamics that drives the system towards a state that maximizes Tsallis entropy with entropic index

ν . See also [1] where this is discussed in more detail. Note that Levin and Pakter do not contest this statement.

We also show that, for a class of systems of overdamped particles interacting through repulsive short range potentials, the particle-particle interaction induces a flux of particles proportional to gradient of concentration. This has been previously discussed in few other works [2]. We also show in our letter that this kind of dynamics leads to a non-linear diffusion equation that is equivalent to the non-linear Fokker-Planck equation (1). Again, none of these statements were contested by Levin and Pakter.

In view of the aforementioned facts we are rather surprised that Levin and Pakter, without contesting any of the results demonstrated in our work, still question our conclusion that “*a system of overdamped particles at $T=0$ is a physical realization of Tsallis thermostatics with entropic index $\nu = 2$.*” This is specially puzzling when one notes that the solution proposed by Levin and Pakter for this system also corresponds to a solution of our non-linear equation, therefore confirming our findings.

2) Levin and Pakter state that our result for the density profile of particles “has everything to do with Newton’s Second Laws”.

Of course, we were by no means expecting our results to be in contradiction with Newton’s Laws. As a matter of fact, in our study, we used molecular dynamics to integrate equations of motion and obtain numerically density profiles, validating the predictions of our non-linear Fokker-Planck equation. In the same way, neither the Maxwell-Boltzmann nor the Tsallis thermostatics are contrary to classical mechanics theory, as one could infer from the final and rather misleading sentence of the comment.

3) In their comment, Levin and Pakter also state that “prior to discarding the standard statistical mechanics one should see what it has to say on this matter.”

Again, our approach and results neither disregard nor contradict “standard” statistical mechanics. We would surely welcome any other treatment based on the solid principles of Statistical Physics. This is definitely not the case of this comment. Instead, however, they categorically dismiss our elaborated approach and results without

employing any concept or tool of Statistical Physics.

4) Levin and Pakter write in their comment that “if a classical system is placed in contact with a temperature reservoir at $T = 0$ it will loose all its kinetic energy and collapse to the ground state” where the “net force on each particle vanishes.” They also state in their comment that “this is precisely what happens for the system studied by Andrade et al.”

Of course, the overdamped motion we investigate should evolve towards mechanical equilibrium, as we explicitly state in our paper. One should note, however, that our approach goes beyond the prediction of a stationary state, since the non-linear Fokker-Planck equation introduced by us also describes the dynamics of the system, with all its transient features, till the stationary regime is eventually reached. The analytical solution provided in the comment, however, does not contemplate the dynamics outside the stationary state. The fact that our system evolves towards mechanical equilibrium is in no way contradictory to our results or conclusions.

It is important to point out that we also studied the case in which thermal noise disrupts equilibrium, resulting in a density function that is consistent with a mixed entropy combining Tsallis with entropic index $\nu = 2$, and Boltzmann-Gibbs formulations, namely, Eq. (17) of our paper. By varying the ratio between the particle-particle interaction and thermal force, we can continuously interpolate the density profile from a Gaussian, at higher temperatures, to a parabolic shape at $T = 0$. In short, what we proposed in our paper and the authors of the comment appeared to miss completely, is that overdamped particles interacting through a wide family of potentials follow a dynamics that drives the system to the maximum value of the entropic function Eq. (17) of our paper. The usefulness of our approach is that the density profile can be easily obtained with the many-body interactions being carried into the entropic term with $\nu = 2$.

5) In their comment Levin and Pakter claim that they obtain an exact solution for the particle distribution at $T = 0$.

This claim is misleading to say the least. In fact, in the same way as we did, their solution is a coarse-grain approximation, where the discrete nature of the interacting particles is replaced by a continuum description for the particle density. However, it is relevant to note that they arrive at a solution that is similar, but not identical to ours. Levin and Pakter also show results from numerical simulations that seem to agree with their solution. As we show here, however, they studied the system at a regime that is significantly different from the one we investigated in our work, and that is clearly the reason for the discrepancies.

The equation of motion for our system of overdamped

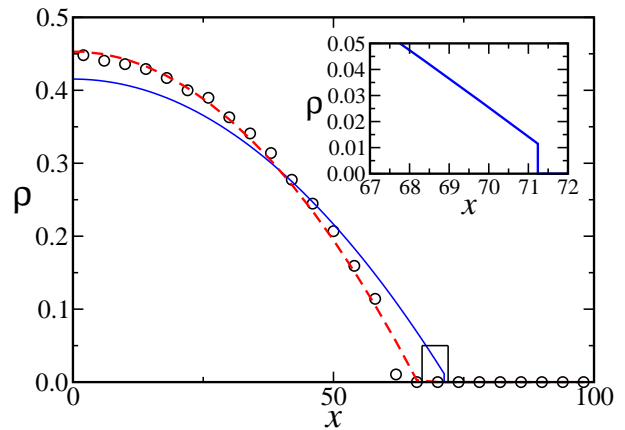


FIG. 1. Numerical results for the density profile obtained from numerical simulations. We used the same conditions as Fig. 1 of our letter, namely, $N = 800, \alpha = 10^{-3}f_0, L_y = 20\lambda$. The dashed red line is the solution for this profile given by Eq. (14) of our letter, while the straight blue line correspond to the solution according to Eq. (3) from Levin and Pakter’s comment. Clearly their solution is not compatible with the concavity of the density profile obtained from our molecular dynamics simulations. The inset shows that the discontinuity in their solution is barely noticeable for these physical conditions.

particles is given by

$$\mu \vec{v}_i = \sum_{j \neq i} f_0 K_1(|\vec{r}_{ij}|/\lambda) \hat{r}_{ij} - \sum_i \alpha x_i \quad (2)$$

where K_1 is the modified Bessel function. The factors f_0 and α control the intensity of the particle-particle interaction and the confining force, respectively. In their results Levin and Pakter used $\alpha = f_0$ while we used $\alpha = 10^{-3}f_0$. It is not surprising that, by using a confining potential 1000 times stronger, one should obtain results that are distinct from ours.

6) Levin and Pakter arrive at a result that is similar to Eq. (14) of our paper, namely, Eq. (3) of their comment. They claim, however, that their solution does not need any adjustable parameter, while we had to set the value of the parameter that controls the concavity of the distribution to $a = 2.41f_0\lambda$, in order to adequately fit our model to our results from molecular dynamics simulations.

Apparently the authors of the comment did not realize that their proposal of $a = \pi f_0 \lambda^3$ as a “non-adjustable parameter” corresponds exactly to the same theoretical prediction presented in our paper. However, this prediction is valid only when the density of particles ρ varies slowly within the interaction range of the potential. Since the system is composed of discrete particles, the value of the parameter a should approach $\pi f_0 \lambda^3$ as the density grows. Since Levin and Pakter use a substantially stronger confining potential, the obtained densities can reach 5 particles per λ^2 , which is about ten times higher than ours.

At this regime, the value $a = \pi f_0 \lambda^3$ represents indeed a good fit to the numerical data. At moderate densities, however, the numerical results do not follow this prediction. In Fig. 1, we show our numerical results and parabolic predictions with $a = 2.41 f_0 \lambda^3$ and $a = \pi f_0 \lambda^3$. It is clear that the value of a used by Levin and Pakter do not agree with numerical results from molecular dynamics in this regime.

In any case, it is important to emphasize that the interacting potential in the form of the modified Bessel function is originally motivated by applications in the theory of superconducting vortices. It is a known fact [3] for this physical system that such a potential form represents an approximated model that is only valid in the regime of moderate magnetic fields (i.e., moderate particle densities, $\rho \ll \kappa^2/2\pi\lambda^2$, where κ is the Ginzburg-Landau parameter). In the case of higher densities, the vortex cores might get so close to each other that can no longer be treated as point-like defects. As a consequence, Ginzburg-Landau equations should be employed instead, meaning that the physical conditions implied by the results of Levin and Pakter should be considered with caution in the context of the Physics of interacting superconducting vortices.

7) Levin and Pakter predict the existence of discontinuities at the edges of the density function ρ .

We would like to point out that, although the numer-

ical results of Levin and Pakter follow their theoretical prediction in the bulk of the density profile, by just looking at their histogram it is not clear whether the discontinuity is really present or not. The inset of Fig. 1 shows that under the conditions of our simulations, this discontinuity, if present, would be negligible. Therefore, we conclude that, in the conditions we model our system, the discontinuity predicted by Levin and Pakter does not represent a relevant effect. In any case, the analytical result of Levin and Pakter can be viewed as a solution of Eq. (13) of our letter, with $a = \pi f_0 \lambda^3$, but subjected to a different boundary condition. Therefore, our understanding is that their results are also consistent with our theoretical model.

In summary, based on the preceding comments and remarks, it is our opinion that the comment by Y. Levin and R. Pakter is neither conceptually sound nor relevant.

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